# REMARKS

ON

# 'HE PRESENT STATE OF THE THEORY AND PRACTICE OF MEDICINE."

BEING

# A REVIEW

OF

## OFESSOR BENNETT'S PUBLISHED INTRODUCTORY LECTURE,

(UNIVERSITY OF EDINBURGH, SESSION 1855-56).

COMMUNICATED TO THE HUNTERIAN MEDICAL SOCIETY OF EDINBURGH, (JAN. 11, 1856),

BY

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"GRAU THEURER FREUND IST ALLE THEORIE."—*Goethe.*

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# REMARKS, &c.

Mr. President and Gentlemen,—

It appears to me that the principles on which this Society is constituted—the utmost rational freedom of discussion, and perfect professional equality of membership—offer opportunities, not so patent in most other Medical Societies, for the examination of received opinions, as well as the statement of independent views, in medicine. I venture, therefore, to submit to you the following criticism of Dr. Bennett's published Introductory Lecture; and I do so in the confidence that any freedom, and it may even be severity, of comment, into which, from the nature of the undertaking, I shall be led, will not be mistaken, by you at least, for the mere arrogance of the critic. It is because I conscientiously differ from Dr. Bennett,—because I consider the scientific position he would assume for modern medicine unphilosophical and untenable—no less than because I think his lecture a narrow and illogical production—that I embrace this opportunity of indicating what I believe to be some of the specious fallacies, the barren speculations—the *ignes fatui* of modern medicine.

It is seldom that so good, or at least so available, an opportunity offers; for the vast majority of medical writings, on account of their bulk, defy the passing criticism of an hour,—their size, if not their strength, doth laugh a siege to scorn; and with what falls orally *ex cathedra*, I, of course, have no title to intermeddle. But when an Introductory Lecture is printed and indiscriminately sold, it becomes the property of the professional public, and there can be no presumption—as there is no difficulty on account of its bulk—in subjecting it to the process of

review, favourable or not favourable to its pretension, in the present instance, as a philosophical sketch of the scientific position and prospects of medicine.

I shall therefore treat the Lecture before us—as I am entitled to do—as if it were the production of some unknown Dr. A., instead of Dr. B., the Professor of Physiology in this University. We shall in fact adopt, for the nonce, an editorial style, and proceed as candidly and as fearlessly as if our notice was intended for the pages of an ordinary Magazine.

Dr. Bennett begins by drawing a distinction between the exact and inexact sciences, and by assigning to medicine a place among the latter; but in many succeeding parts of his lecture he loses sight of this important distinction, and in the most reckless manner subsequently speaks of medicine, now as if it were an exact science, again as a mere art. To those who have scanned much of our modern medical literature—not so much in the expectation of finding new facts there, as in the vain hope of lighting upon some tangible philosophy of medicine—this confusion of ideas, this constant shifting of the ground on which it is proposed to build a much talked of Rational Medicine, is not new or surprising. It is a confusion not confined to any writer in particular, but seemingly almost universal. Nor is it surprising; for when we come to analyze any of the definitions of medicine usually propounded, with one exception, we find them untenable, and nearly useless as starting points.

To define medicine as an 'inexact science,' and, in the next breath, to speak of it as a 'healing art,' seems utterly inconsistent. For the first is much too general a definition, and will not bear analysis; the second is perhaps nearly correct; but if so the scientific pretensions of medicine are at once disposed of, and it thenceforth takes its stand on simple observation and experience. Yet this is the kind of oscillating position which medicine, as represented by its literature, holds at this hour: it is scarcely possible to peruse a medical work, that professes to deal with principles, without encountering a lamentable logical confusion of this nature.

As we have said, Dr. Bennett's first definition of medicine—that of 'inexact science'—will not bear analysis: For it would

be necessary to show, of the separate branches of inquiry that make up the compound whole of medicine—that each of them separately is an inexact science also, or that they all agree together in constituting such a science. But this is not the case, for, according to Dr. Bennett, one at least of the branches that go to make up the so-called inexact science of medicine—Chemistry, to wit—is itself an exact science; so that a part is greater than the whole, which is absurd. The definition is untenable also in the opposite direction, for besides chemistry, anatomy, physiology, materia medica, and botany, which are popularly known as the medical sciences, observation and experience are confessedly indispensable to the existence of medicine: but inasmuch as observation and experience are common alike to the savage and the physician, inasmuch as they are, in themselves, not sciences in any sense,—so it follows that, in as far as medicine rests on observation and experience, it is not in any sense a science.

To what extent medicine rests on observation and experience is a further question. In our opinion it rests upon them primarily, mainly, almost entirely: but we have no direct excuse for defending such a thesis at present. It were not difficult to show, however, that Dr. Bennett virtually, though of course quite unintentionally, admits as much;—for, having defined medicine an inexact science, and given as the distinguishing characteristics of exact and inexact science, that the one does, and the other does not possess a primitive fact or law, "medicine"—says he, page 6—"in its present state, possesses *no* primitive fact." Now if medicine possesses no primitive fact, on what, we might ask, *can* it fall back else than observation and experience?

Our lecturer, indeed, takes refuge in the future, as many have done before him, and ventures on the following prediction:—

"One day another Newton may arise, whose genius will furnish *our* science with *its* primitive fact . . . . Although it must be confessed that we have not yet arrived at such a happy consummation, it cannot be denied that we are making rapid strides towards it."

On the contrary, we shall have occasion, before we have done, to suggest such doubts as amount to a decided denial of what Dr. Bennett here assumes. In the meantime, we have only to observe that, from such a point of view, a definition of medicine

is simply impossible. If it be an inexact science, characterised by possessing no primitive fact, we are not entitled to say—because it does admit of proof—that it is 'making rapid strides' towards that very primitive fact, the absence of which is essential to its definition.

The inevitable consequences of all attempts at a scientific definition of medicine—that is to say, confusion, and a change of ground—are at length met by the admission that medicine is *an art:* Dr. Bennett says, at page 9 :—

"I trust that, in studying this, (Physiology,) as all other subjects, you will never lose sight of the important fact that you are medical students, and that as such, your ultimate object is to acquire an art, in other words, a knowledge of all those means which are directed to the prolongation of life and cure of disease."

This is quite intelligible. The practical result, the sum of the compound studies of medicine, is a *healing art:* as we have already hinted, no other definition is tenable. But it is inconsistent with the scientific position formerly claimed for medicine; and as if feeling it to be so, after insisting on the value of *theory* in the practice of the *art*, Dr. Bennett proceeds to distinguish '*carefully*,' as he says, "what is science and what is art," thus (page 11) :—

"We may consider then, science to be a collection of theories; art a body of rules. Science says, this is or is not; this is probable or improbable. Art says do this, avoid that. The object of science is to discover facts and determine laws; the object of art is to accomplish an end, and to determine the means of effecting it. Science is inductive and reasons; art is imitative and exemplifies."

In the above paragraph of definitions the lecturer riots in a loose use of terms, the exact or positive, and the inexact or speculative sciences being confounded: what he means may or may not be true, but what he says is undeniably incorrect. Are the mathematics, for example, a collection of theories?—is astronomy, founded on mathematics, a collection of theories? Moreover, science is not necessarily inductive, nor does it necessarily reason; on the contrary, exact or positive science is deductive, and it does not reason, it demonstrates. But this last kind of science repudiates medicine, and hence much of the confusion in which the champion of a would-be scientific medicine inevitably becomes involved.

What, however, Dr. Bennett means—we take it—is, that there is an art as well as a science of medicine. But he means this evidently, not of medicine *as* medicine, but, of some one or other of its branches or practical applications; and his subsequent "striking examples" of how far the one is dependent on the other—i. e., art on science—partake, as might be expected, of the inconsequence attaching to the definitions they (viz., the "striking examples") are meant to illustrate. This we go on to show. Meanwhile let us grant the lecturer's dictum, that science is a collection of theories,—in other words, that *science* in medicine is synonymous with *theory*. We shall peradventure discover, by and by, in following him farther, what is, according to him, the practical value to suffering humanity of such a collection of theories.

"Science," he says, p. 11, "in numberless instances, has advanced beyond art; nay more, science herself has worked out all the details, and made art a mere slave to her commands. Thus it was that the theory of achromatism, worked out by Euler, led opticians to make perfect telescopes and microscopes."

In this, the first of his 'striking examples of how far art is directly dependent on science,' Dr. Bennett refers to the laws of optics *merely* to prove that science has "advanced beyond art:" he has not brought the illustration to bear on medicine in general, or physiology in particular. Useless in his hands, it may be of some account in ours; we shall show how it tells if honestly brought to bear on that branch of medical study with which he is more immediately connected—physiology.

The laws of optics refuse to be turned to physiological uses. The principles on which the human, and all other fully developed eyes appear to be constructed, are strictly scientific principles, and we are indebted to mathematics for their discovery, and their application to mechanical instruments, long before their explanation of the phenomena of vision was suspected.* But the instant we attempt to apply mathematical principles to the optical theories of physiology, we come to a stand-still. Of what value are the several theories that have been advanced in explanation of the wonderful fact—That, while we possess the

* Hence it is not correct, at least in this instance, to say, with Dr. B., that "art for many ages preceded science."

faculty of correct vision, the retina offers us an inverted image of every object we behold? Towards reconciling this apparent contradiction, the most various and mutually destructive theories—physical, metaphysical, and physiological—have been proposed by natural philosophers, as Sir David Brewster and others, on the one hand, and authoritative physiologists, as Müller, Volkmann, and Carpenter, on the other hand. The cause of single vision with the two eyes has given rise to equal, or greater variety of solution: at least five conflicting theories have been propounded by physiologists—Let us hear no more of the theories of optics in the interest of scientific medicine (so-called). But again—

"Thus it was that Le Verrier and Adams, by calculations in their observatories in Paris and London, discovered a planet which they had never seen, but which, when looked for according to their directions from Stockholm and St. Petersburg, was immediately proved to exist in fact, as it had previously been proved to exist in theory."

Now, to say that this planet was discovered before it was seen is to use very loose and unscientific language: It was merely *calculated* before it was seen; and although the calculation, being a mathematical one, was absolutely correct (and all mathematical calculations are necessarily either absolutely correct or absolutely incorrect), yet its observation, and nothing less, was the proof of its existence as a planet.* Of course it could not be "proved to exist in theory" before it was "proved to exist in fact;" its existence could only be rendered probable in theory, because *probability* is the utmost proof of which theory is capable,—a fact that stares Dr. Bennett's definition of science rather awkwardly in the face. He should have rendered this planetary example really "striking," by furnishing a corresponding example of a medical theory founded on a mathematical calculation: he has not done so.

We pass over his third illustration, wherein "the electric telegraph, perfected in the closet of the man of science, flashes

* In the opinion of its discoverer (as a planet), Leverrier, Neptune is identical with the star of the eighth magnitude observed by Lalande on the 10th May, 1795, and marked in his catalogue of the fixed stars. So far, therefore, from this planet having been first proved to exist in theory, it was actually *seen* fifty years before it was dreamt of in theory: Dr. Bennett, we suspect, was ignorant of this fact, or he would not have employed the illustration.

ready made on the astonished gaze of an admiring world," because it will again flash on *our* astonished gaze; and now we come to another, and it is the last of the "striking examples" intended to show, in a medical direction, the dependence of art on science.

"The art of navigation," says he, p. 12, "is certain, because the science of astronomy on which it is based, admits of exact calculation; and, in consequence, the tempest-tossed mariner, although in unknown seas, may, by his instruments, ascertain the exact spot his vessel occupies on the surface of the globe. In like manner, the only way of improving the art of medicine is to advance the science of physiology, and all that has been accomplished during the last fifty years has been brought about in this manner."

Nothing can well be more untrue than what is here assumed, to wit, that mathematics and astronomy on which navigation is based, and physiology, on which, for the nonce, medicine is held *in like manner* to be based, are sciences of a similar kind; and we might quite as fairly give to some other branch of medical study the isolation here claimed for physiology: we might quite as justly say, that "the only way of improving the art of medicine" is to advance chemistry, or anatomy, or the materia medica.

We freely grant what the lecturer adds, viz., that the discovery of "the independent properties of the nerves, the reflex functions of the nervous centres, &c., the laws regulating the development of the ovum, the significance of the sounds produced by the heart and lungs, and numerous other doctrines" —some of them chemical, some mechanical, some anatomical, some derived from natural history, some from simple observation—"have tended to improve the art of medicine." It by no means follows that they have rendered, either physiology in particular, or medicine in general, a science similar to astronomy. We shall presently see, as far as Dr. Bennett can let us, what these numerous doctrines have effected for the relief of suffering humanity.

"But let me descend," says he, p. 12, "from generalities to a few striking instances of what a study of the theory of medicine has recently acomplished for the improvement of its practice."

We proceed now to examine these "striking instances," as we previously did the other "striking examples:"—

"From very ancient times, the salt lakes of Fusaro have been used as nur

series for young oysters. It has been calculated that each female oyster produces, on an average, about 100,000 young, which, when they escape from the shell, appear like a thick white cloud in the water. Each minute particle is at first furnished with cilia, by means of which it swims about till it finds a surface to which it attaches itself. If this cannot be found, the cilia soon fall off, and the young animal sinks to the bottom of the water, where it becomes a prey to the polypes which are fixed there. Now, at Fusaro, it has been found that if fagots, or the dried branches of trees, are driven into the sand, so as to present to this animated dust a surface to which it may attach itself, the minute particles cling to it, as a swarm of bees cling to any projecting substance. There they become fixed, grow rapidly, so that at the end of two or three years each minute ovum becomes an eatable oyster. Then the fagots or branches are pulled out, and people gather, at Fusaro, a harvest of these shell-fish annually, in the same way that, in other places, they gather grapes." . . . . . It has long been known that the salmon-fisheries in this country have been annually becoming less and less productive, so that what was formerly the food of the common people has become an article of luxury for the rich. Now, the experiments of Spallanzani demonstrated that a minute quantity of the male spermatic fluid, when diffused in water, would impregnate a multitude of ova obtained from the row of the female frog, when sprinkled over them. This important fact has, within the last few years, been taken advantage of, with a view of producing artificial fœcundation," &c.—Pp. 13, 14.

After reading the above, we may well ask what the researches of naturalists into the mode of propagation of the oyster—what the trite experiments of Spallanzani on the ova of frogs, have directly to do with the matter in hand. How these researches and experiments constitute a "striking instance of what a study of the theory of medicine has recently accomplished for the improvement of its practice," it puzzles us, we confess, to comprehend; and, with all his ingenuity, the lecturer himself cannot—at least he does not—tell. He says, indeed, at the beginning of the succeeding paragraph—

"Here, then, is an example of how physiology has been made useful in procuring food, in increasing property, and in adding to our national resources."

True enough; something or other has been made very useful indeed in procuring food, increasing property, and adding to the national resources,—but not by any means physiology in particular: Might we not say as much of agriculture, of mechanics, of each of the arts, and of civilization in general?

Equally inappropriate is the next instance (page 14):—

"The upper line of diagrams before you points out the mode of development of one of the higher mammals—the dog. If we were to compare the matured ovum at one end of the line with the nearly completed fœtus at the other, the mind might well be puzzled to determine by what steps the one was transformed into the other. But by tracing the successive changes which take place, we observe, how by a series of involutions and evolutions of a membrane, called the

germinal membrane, how by its splitting up into three layers, each becoming thickened, and how, by then undergoing a number of turnings in and bendings out, the perfect creature is formed. Now, we do not say of such a development, the whole of which is hid from us, that the different stages constitute different animals; but in those cases where the progress is not hid, strange to say, this is what has resulted."

He might have added—what would have been at least a shade nearer his presumed text—that the human fœtus undergoes similar metamorphoses; embryotic man being, as Meckel says, distinguished from the other animals by the greater rapidity with which he passes through the inferior formations; but unless it were to show that he was well versed in embryology in its bearings on physiology, what could the lecturer possibly mean by introducing such facts in such a connection? How do they show "what a study of the theory of medicine has accomplished for the improvement of its practice?"

He next alludes to the researches of comparative anatomists into the habits of certain parasites that infest the viscera of various animals, asserting that the modern treatment of tape-worm in the human subject constitutes one of the triumphs of theoretical medicine (page 16):—

"Man, like other animals, is apt to be infested by a peculiar kind of tape-worm called *Tænia Solium*, and there can now be no doubt, that this form of entozoon is a further stage of development of the *Cysticercus Cellulosæ*,* so common in oxen, sheep, and especially in pigs. These animals are largely consumed by man, and where their flesh is eaten raw, as in Abyssinia, tape-worm is very common. In civilized countries, cookery destroys the vitality of the parasites before they are eaten; but if meat be underdone, or eaten out of season, a few may escape the action of cooking and of mastication, and so reach the stomach uninjured. That dogs fed on rabbits, or mutton flesh containing *Cellulosæ*, become affected with tape-worms, has been proved by the direct experiments of Kuchenmeister.

"But, you may ask, how do all such facts, interesting though they be scientifically, teach us to cure disease? At first, indeed, this is not apparent, any more than was the use of the latent theory of heat as developed by Black, or the utility of exciting spasms in a frog's limbs, by bringing them in contact with two metals, as was done by Galvani. But the theory of Black led to the construction of the steam-engine, and the barren observations of Galvani have enabled us to unite France and England by a telegraphic wire through the ocean. And, it may be assumed, without the fear of contradiction, that it is such facts as I have alluded to, that are silently revolutionizing the study of medicine. Thus, if we want to cure tape-worm, it must be clear that it is not enough to give anthelmintics or purgatives; we must also prevent the eating of flesh underdone, or game and fish out of season, when it is likely to be infested with *Cysticerci*."

* Dr. Bennett seems to have forgotten—otherwise he would naturally have mentioned it here—that the *Cysticercus Cellulosæ* is itself found also in man.

This is the third "striking instance," and far fetched as we shall show it to be, it yet has *some* presumed connection with the subject; it makes some effort—however futile when examined—at showing "what a study of the theory of medicine has accomplished."

In the first paragraph of the above quotation, some facts are merely stated: in the second, we have an example of false analogy and inconsequent reasoning—a kind of fallacies already more than once encountered in the lecture under review—in the present instance reminding us, we confess, of the fabled mountain in labour which brought forth a mouse. Steam and electricity, the two greatest wonder-workers of the age, are, as it were, too good to be lost as illustrations: no matter how, they must be got in. They are enlisted in the service of theoretic medicine accordingly, and lo! the tape-worm is evacuated by steam, and so effectually electrified, that the patient is troubled by the astonished parasite thenceforth nevermore!

While we allow that the treatment of tape-worm, which, besides giving anthelmintics, prohibits the eating of half-cooked animal food, reflects credit on those whose observations led to it, we entirely dissent from the assertion, that "such facts are revolutionizing the study of medicine;" and we repudiate the analogy of the steam-engine and the telegraphic wire. The observations alluded to (of Kuchenmeister and others), were made on mice, cats, pigs, dogs, &c., for the detection of certain species of cysticerci, tracing them from lower to higher conditions and habitats up to man. On the other hand, the experiments of Black, Galvani, and their followers—which ultimately led to the construction of steam-engines and electric telegraphs—were made with elements and forces totally different—the imponderable forces, heat and electricity. The things compared, therefore, are not merely different in kind, but the results as disproportionate in degree.

Locke, in the early part of his career, was a physician, and we may be sure that—however ignorant of *cysticerci*, and other parasites—when he reasoned as a physician, he did so logically. Suppose Dr. Locke were to revisit this sublunary scene, and, when viewing for the first time the action of a steam-engine, or

he working of an electric telegraph, suppose he were to be old, by some modern medical theorist, that the discovery of the habits and transmigrations of some entozoa, and some little improvement in the treatment of tape-worm since his day, are qual in importance, as scientific discoveries, to the results of team and electricity—we may imagine how the resuscitated phipher would stare. "Sir," he might possibly reply, "as for he comparison you institute between things so different in kind nd degree, I do not pretend to understand you; unless indeed ny suspicion be correct, that however physic has advanced in heory, some of its professors have not improved as to their reaoning powers, or their common sense, since I practised as a hysician at Oxford nearly two hundred years ago. As for the ape-worm, we were familiar with that parasite, and the means f expelling it from the human intestines in my day. By adainistering a dose or two of the bark of the pomegranate-root a remedy at least as old as the time of Celsus), we cured our atients, sometimes permanently, sometimes not; and the tenency of the complaint to recur sometimes puzzled us. But, at east, I can bear testimony to the truth of what you say about he importance of advising subsequent attention to the diet of uch patients; for it was matter of observation and experience, o long ago, that the parasite was least likely to trouble again hose who abstained from fish, game, or other animal food, imerfectedly cooked or out of season."

We have next an instance of a vegetable parasite, in the skinisease popularly known as "scald-head," medically termed *Tinea Favosa*. The microscope having demonstrated that the rusts (or scabs) which cover the scalp in this affection, structurally resemble certain of the fungi, an appropriate theoretical reatment follows as a matter of course, which if neither very satisactory nor original, as we shall show, is at least a vast improvement on the process of the *calotte*. Many barbarous practices prevailed formerly, and it would seem that scalping was not so xclusively confined to the Red Indians as our school-boy noions had given us to understand. We are told, page 17, that—

"Formerly this disease was attempted to be cured by plucking out the bulbs f the hair with a pair of pincers, or by the barbarous practice of the *calotte*.

This consisted of spreading a thick cohesive plaster over the shaved head, and when the hair had firmly grown into it, dragging it off forcibly, and so endeavouring to eradicate the disease and hair together. . . . . But the discovery of the vegetable nature of this disease rendered it evident, that only those means hostile to vegetable growth would answer the purpose. For several years I have accomplished this, by simply shaving the head, and keeping the scalp smeared with oil, which, by preventing the access of atmospheric air, at once destroys the conditions necessary for the development of fungi."

The *calotte*, we suspect, went out with the last of the barber-surgeons; but there was another class of practitioners who professed to cure scald-head by milder applications, rivalling the famous oil of modern theoretics. Old women existed formerly—they exist yet—with faith in soap and water—especially soap—and some of them have still the audacity to affirm that they can cure "scald-head," and eke the itch, by means of perseverin lather. And why not? The barber-surgeons, the scalpers, the *calotte* practitioners, doubtless sneered at the old women of the day; but the old women were right, and Dr. Bennett has proved it. Perhaps, indeed, he has proved too much, for soaps, as we all know, are just oils with potash and soda added, and these alkalies having been found useful, and being medically accredited as separate applications in some skin affections, it may plausibly be argued that the lather of the old women is theoretically (ay, even theoretically) preferable to the pure oil of Bennett.* Sooth to say, neither is a remedy much to boast of in *Tinea Favosa*—an affection mostly confined to childhood and youth, and for the cure of which time often does more than any remedial measure; though wicked experience has been heard to exprate of the frequent good effects of such applications as the iodide of sulphur ointment, but of course on no satisfactory theory whatever.

In connection with this subject of skin affections, a word about scabies (vulgarly, the itch), to which the lecturer alludes in a similar strain at page 22.

* We are far from anxious to underrate the therapeutic virtues of oil, especially the cod-liver oil, when internally administered in incipient phthisis, and the strumous diathesis generally. Neither Dr. Williams of London, however, nor Dr. Bennett of Edinburgh, both of whom claim the merit of its reintroduction in practice, would deserve much credit, even could he make that claim good, compared with the obscure country practitioners, and sensible old women of our own sea-coast villages, and those of the continent, with whom it found acceptance and a refuge when it was slighted or forgotten by all the Colleges of Physicians.

Most people know that soap and water suffice, in many cases, to cure scabies; and every medical man is aware, that the disease depends on the presence of an insect, the *acarus scabiei*: "But," says Dr. Bennett, "to discover these insects, and to determine their habits, patient and long-continued research was necessary, by means of the microscope, and practice now reaps the benefit of it."

If Dr. Bennett means his kind of practice—the pure oleaginous treatment of scabies—then we say, decidedly, *No*, practice does *not* reap any benefit, but rather gains a loss; for those cases which do not yield to scrupulous cleanliness are certainly not curable by oil, and the itchy patient, who lubricates his hide, does but lose time and labour. As there is no denying the fact that the oleaginous treatment often fails, recourse is had to a theory to explain the failure, as if the failure were not sufficient, or the object was to make fact obedient to sacred oil—and we are told, that—

"In chronic cases the eggs, however, remain, and hence other applications may sometimes be necessary," &c.

Now, one would suppose that, if oil can asphyxiate the *acarus* when it comes out of the egg, no other application should be necessary to cure any form or stage of scabies; unless, indeed, the said eggs are a vast deal longer in hatching than startling facts lead us to believe. The fact remains, however, in spite of "the patient research by means of the microscope" of which we hear so much, that, in serious cases of scabies, we are obliged to fall back upon an old wife's remedy once more, and to accept, with as good a grace as possible, the nasty, empirical sulphur ointment!

So much for the kind of facts as to which Dr. Bennett gravely says, "it may be assumed, without fear of contradiction, that they are revolutionizing the study of medicine." (!)

There is something that would be ludicrous, if it were not also mournful, in the peep-show magniloquence with which these microscopic wonders—these liliputian attainments are trumpeted forth to a generation of mankind asking, not for such a kind of glad tidings, but for one grain of hope in the prospect

of another visitation of Asiatic cholera, or, it may be, some other disease as new and fatal, before which, when it suddenly comes, theoretical medicine will, not for the first time—stand silent, or retreat appalled. Whence all this mere varnish for outward application? What avails all this pompous jargon about scab and itch, while we have not one fixed principle to guide us, not one ascertained remedy to arm us, in the treatment of those sudden and awful epidemics which we have seen before, and will assuredly see again, decimating a population calling to us in vain for help—reproaching us, as it were, with their last breath, haunting us with their dying looks? Away with your scabbed-heads and itchy extremities! Science, you say, is revolutionizing medicine,—theory, you affirm, is confounding blind experience; give us, we pray you, serious proof of it. Give us some more evident, some less microscopical fruits of your glorious theory and your omnipotent science. Teach us how to vindicate their majesty, insulted by an incredulous, a quack-ridden, a pestilence-smitten public;—tell us how we are to answer the professional sceptic who points exultingly to Vienna homœopathic statistics, to French expectant treatment reports, in proof of the alleged fact, that, when it comes to the push, when such diseases as typhus fever and Asiatic cholera become the test of power in practice, results confound us all. In a word, can your theory, or even your microscope that reveals so much about scab and itch, do any thing more for us,—can it teach us to save life? This, we take it, is the prime question for us. Science—*genuine* science—is in no danger of being mocked; she will ultimately vindicate herself, but death will not wait. We ask you again, are you willing, are you anxious to be judged by your fruits—and if so, where are they? Alas! in this lecture of Dr. Bennett's, the professed object of which is to set forth the triumphs—the practical triumphs over disease, of theoretical medicine, we can find only such fruit as crumbles to dust when we touch it: as the weary traveller, already sick of mirage, standing on the blasted shores of the Dead Sea, is mocked by the painted apples of Sodom!

In strange contrast with this exaggeration of insignificant observations, this inflation of paltry results in practice, Dr. Ben-

nett subsequently alludes (page 19), with apparent indifference, to one of the few important observations involving theories of disease, which are really, in some sense, tending towards practical revolutions in medicine. He denies the truth of the observation made by Dr. Alison, and other observers of eminent experience, that some inflammations, like fevers, have changed their type since the days of Cullen and Gregory; and having denied it, he dismisses the question as of no further consequence.

To be sure it does not admit of microscopical investigation; but supposing it likely to be true (and the evidence for its truth is so remarkable, that prejudice only can venture to ignore it), how infinitely does such an observation as this transcend improved treatment of tape-worm, oleaginous application to scalp of dirty boy, and eke theoretical smothering of acari! Seems it not amazing—where so much previous ingenuity had been exercised in ferreting out the microscopic ills exceptional skin is heir to—that this vital and comprehensive speculation (supposing it no more in the meantime), should be so lightly jumped over? If found to be correct, what an important fact, in the history of disease on the large scale, is this on which Dr. Bennett turns his back! of what consequence to mankind that physicians should determine, one way or other, the alleged fact. Dr. Alison may possibly be right. Why not? If it be true—and it *is* true—that disease has undergone vast historical revolutions, why may not special diseases change their type?—It is at least natural and likely that they should.

It is matter of undoubted fact in the history, not so much of medicine alone as, of the human race, that great diseases have undergone great revolutions; that certain of them have disappeared, and that new ones have appeared from time to time amongst mankind. Where now is the true leprosy, once the most loathsome and the most dreaded pestilence of the civilized world? It is confined to Eastern India and the extreme north of Europe, where even it is on the decrease. Syphilis, which appeared as the successor of the true leprosy in Europe, and which is therefore not a very old disease, seems, in its turn, destined to give way ere long to a new disease, perhaps generically allied to it, and, may be, even shorter lived. As a European disease, sy-

philis seems to be on the wane; in its most malignant form it i now comparatively rare, and in no form can it be said to rag as a pestilence. On the other hand, the Asiatic cholera is quit a new disease; so new, and so little understood that, for al purposes of cure—nay, of palliation even—the most accom plished and experienced physician, who finds himself face t face with it, seems powerless as its victim.

Yes, it is at such a test as this—in the presence of a new an terrible disease—infections, coming suddenly, and sweeping o multitudes of mankind ruthlessly, yet as it were capriciously that Theoretical Medicine, with all her pretended scientifi attainments, stands dumb, confounded, and ashamed, hiding he microscope in its proper corner! By and by, the pestilenc having overpassed, she reappears, microscope in hand, and tell us, perchance, that (page 22) :—

"All these contradictions depend on imperfect attempts at correct theory and this latter once rendered perfect, it will be seen that both health and disea are governed by laws as determinate as the motion of the planets, and the cu rents of the ocean."

So says discomfited Theory, putting on the greasy mask o prophecy; and were like things compared with like, perchanc we might be induced to accept so flattering a prediction. Th laws that govern the planetary motions and the tides were onc unknown; but, even before their discovery, it was at least cer tain that they were regular and uniform in their operatio Unfortunately, the laws that govern disease—whatever or how ever discoverable they may be—are very evidently the reverse: in definite, changeful, fugitive, as disease itself. They are—the must be, speaking of them as unknown—subject to what dis ease is subject to, atmospherical, climatal, periodical, individua influences; a number of irregular and unpredicable concomitant Yet the laws that govern disease—intangible though they be the air on which they ride—*may* yet be demonstrated, explained brought within the narrow enclosures of medical *surveillanc* The millennium has yet to come. But, at present, it is a litt too much to assume, that the unknown laws supposed to gover disease are *like* the known laws that govern the planetary mo tions and the tides: it is irrational to compare the discover

nd the immutable, with the undiscovered and the mut-
ble.

Having, in the face of historical evidence and living expe-
ience, denied that fevers and inflammations have ever changed
heir type, Dr. Bennett, with seeming consistency (it is only
eeming), ascribes the diminished mortality in pneumonia in
ecent times exclusively to the comparative abandonment of
lood-letting in the treatment of that affection. But, we re-
pectfully beg to ask him, *What led to this abandonment of blood-
letting in pneumonia?* And here we fix him betwixt the horns
of a dilemma. For either it was blind experience, or a theore-
tical demonstration of blood-lettings' *modus operandi* in pneumo-
nia, that led to the change of treatment. If he should say blind
experience, then would he admit what is contrary to his princi-
ples—that blind experience is a good and safe guide in medicine,
since in this important instance it has led to diminished mor-
tality; if a demonstration of blood-lettings' *modus operandi*,
then where is that demonstration? or, if it exist, what is it
worth? But such a demonstration exists not, or, if it exist, is
worth nothing; because it has not yet been determined even
what set of capillaries is specially engaged in the inflammation
of pneumonia.* Therefore, either it was blind experience that

* Dr. Williams, one of the very few writers who has expressly alluded to this question, in his article on pneumonia, in the 'Cyclopædia of Practical Medicine,' observes:—"These examinations and some pathological considerations, induce us to consider the capillary ramifications of the pulmonary artery and veins to be the proper seat of pneumonia, and that these may involve more or less of the tissues through which they pass." This, we presume, is also Dr. Bennett's opinion. A very different, and, we suspect, a juster view, is advocated by Dr. Morehead, in a clinical report of 103 cases of pneumonia, treated by him in the Fejeebhoy hospital at Bombay—a most valuable series of observations on the disease, extending over a period of six years, recorded in the 'Transactions of the Medical and Physical Society of Bombay' for 1854. He says:—"The capillaries of the bronchial arteries are the nutrient vessels of the visceral pleura, of the mucous lining, and other structures of the bronchial tubes, and of the connecting areolar tissue of the constituent parts of the lungs. We cannot avoid the conclusion, that they must also be the capillaries concerned, when the inflammation is of the pulmonary cell walls, and of the areolar tissue that connects the cells to each other.

"The capillaries of the pulmonary artery, on the other hand, convey venous blood to the air cells, to be distributed on their walls, in order that the physical process of endosmosis and exosmosis may take place between the gases of the blood and of the atmospheric air. It cannot be looked upon as probable, that the blood in these capillaries takes any part in the vital processes of nutrition of the cell walls. It is, therefore, a just conclusion, that these capillaries and their

reduced the mortality in pneumonia, or else (there is no alternative), the disease has changed its type since the days of Cullen and Gregory. Dr. Bennett may take his choice.

Let us now turn to the latest statement of a fundamental doctrine in medicine, and see what profit theory derives from the, so called, cell pathology (Lecture, page 17) :—

"Although fifteen years have elapsed since the cell doctrine of growth has been admitted into physiology and pathology, medical men have not yet realized to themselves its vast importance in a practical point of view. Professor Virchow of Wurzburgh, indeed, has recently endeavoured to replace the doctrines of solidism, fluidism, and vitalism, by that of what he calls the cell pathology. But I have taught the cell pathology for the last fourteen years in this school, and have gone further, by showing that it is no more universally applicable to the phenomena of disease than is humoralism or solidism. Indeed, we may more correctly speak of a molecular pathology, as a molecule, and not a cell, is the first and last form of organization. But molecules, in their turn, are deposited from fluids, and so we again arrive at a species of humoralism."

Is it possible! Is it indeed true, that, after so much discourse has been wasted, after so many wordy volumes (upon cell growth) have been given without enlightenment to a gaping professional world, we are not ashamed to retrograde a century or two, and e'en exhume the mouldering humoral pathology of our great-grandfathers! *And thou, too, Brutus!* we instinctively exclaim, after having read Dr. Bennett's victorious peroration, wherein he says,—

"Everywhere we see Natural Philosophy advancing—enthusiastic chemists pushing forward organic analyses—anatomists, unwearied in their researches concerning development and the structure of tissues—physiologists experimenting and concentrating all the resources of modern science, in order to elucidate organic laws—and pathologists busy in connecting the symptoms observed in the living with alterations in the minutest tissues and atoms of the dead. At this time medicine is undergoing a great revolution," &c.

*Vox*, we say, *vox et præterea nihil.* A great revolution indeed not unlike some other revolutions witnessed in these times, resulting in zero—that is to say, in fluid—fluid frozen for a century! Yes, after ages of research, and a sinful waste of time and study, it appears that we are returning to an exploded humoral pathology.

A startling fact this, my friends, suggestive of some surmises

blood cannot be agents in the altered state of nutrition of the pulmonary cel walls, and their connecting arcolar tissue, which we designate by the term pneumonia."

perhaps more philosophical than pleasant or hopeful, for the curious disciple of Æsculapius to indulge withal. Does the history of medicine, like the history of the philosophy of mind, really describe nothing but circles?—looked at nakedly, stripped of the robes of theory, is medicine at length discovered to be the eternal serpent with its tail in its mouth? Few who have curiously perused some of the older medical writers (*e. g.* Paulus Ægineta) have missed the conviction that, beneath a nicer phraseology and buttoned in a tighter dress, we vain moderns are playing, not so much original tragedies or farces, as mere revivals in medicine;—revivals, not alone of old plans of treatment in disease, but of ideas and theories also. So that, often, when modern medical writers profess to deal with principles, or with theories, we can readily find something nearly amounting to a parallel in the ancient authors. In the last page but one of his lecture, Dr. Bennett talks much as a more famous physician did some two thousand years ago, when he says—

"This conviction (to wit, that health and disease are governed by laws of an exact and determinable kind), is now everywhere gaining ground, and the public are beginning to distrust the man who merely boasts of his experience and the action of his drugs, and to place confidence in him who treats according to natural laws, and simplifies his remedies. Even quackery has changed its features," &c.

Asclepoides was much of the same opinion in his day. He also affected to abhor and despise what he vaguely termed quackery (doubtless meaning thereby all the medical cries of the day with the exception of his own gallinaceous cackle), and to undervalue previous experience. He also affected to treat disease "according to natural laws" (yet to be discovered), and to "simplify his remedies." He also reduced the phenomena of disease to a doctrine of atoms and pores, very like, one would say, our latest molecular pathology. He also opposed blood-letting in inflammations (and, of course, in pneumonia); he advocated a tonic regimen in disease generally; recommending gestation, the use of wine, and the external and internal application of cold water, particularly the use of the shower-bath, of which he seems to have been the designer: he made much of the division of diseases into acute and chronic, and had faith in frictions (with oil?). True, the ancient, unlike the modern Asclepoides, had

neither microscope nor stethescope; but it by no means follows, that he was unblessed with an acute pair of natural eyes, nor is it the less likely that he was in the habit of applying his ear to the chests of his patients.

But let us now see what kind of practice our modern Asclepoides would have us found on a retrograde cell pathology (page 19):—

"The growth of tumours may be encouraged or retarded by the same means which influence all kinds of cell development. But, if they assume a parasitic character, as in cancerous growths—that is, if the cells possess a power of multiplication in themselves—then the only chance of cure is in their complete destruction or extirpation. But the surgeon who trusts to his naked sight, forgets that germs are infiltrated among the surrounding tissues. These he cannot see from their minuteness, yet he employs no microscope to discover them. Need we wonder therefore, that they should frequently return, or rather grow again, as in fact they have never been thoroughly removed."

Here, limiting himself to a glorification of the microscope at the expense of operative surgery, Dr. Bennett overlooks a fact with which, of course, he is familiar, to wit, that cancer is a disease of the blood. This fact, though a proof of his wisdom in embracing the ancient humoral pathology, demonstrates the absurdity of the practice he would have surgeons adopt; for if cancer be a constitutional, and not mainly a local affection, it is plain that the microscope can be of little use in the treatment of it. Constitutional remedies, directed to the alteration of the constituents of the blood, are surely the only medical means calculated to ameliorate or cure the cancerous diathesis. As for the external manifestation—as for the cancerous growth or tumour—experience having proved that, under certain tolerably well defined circumstances, this may be removed with much immediate relief, and the prospect of permanent immunity, to the patient—the surgeon removes it accordingly. As a guide to the performance, or non-performance, of such an operation, he has something much better than a microscope to consult; he has, that horror of your affectedly scientific physician, a *symptom*. The surgeon (sadly practical man!) examines his patient's glandular system, and extirpates, or not, the tumour, smiling perchance the while—as well he may—at the harlequin idea of thrusting a microscope into the wound in search of germs which may be "infiltrated among the sur-

ounding tissues." Regarding the tumour itself as a symptom, ie treats, he extirpates that symptom, and thus vindicates the ruth of a practical principle which theoretical medicine, nowadays, affects to contemn; to wit, that it is often necessary, generally wise, to treat diseases in direct relation to their symptoms.

And this brings us to notice one of the lecturer's general observations, wherein he mourns the ignorance and incapacity of his professional brethren in regard to the use of the stethoscope (page 19):—

"Notwithstanding the universality with which the stethoscope and auscultation are now received as necessary means of diagnosis, how few of our medical men, comparatively, are really skilful in detecting by them the morbid changes going on in the heart and lungs."

Now, despite the above modest and affecting reflection, there really never was a time when the detection of diseases of the heart and lungs, so far as can be accurately accomplished by means of the physical signs alone, belonged more to the profession at large, and was less justly the boast of any one coterie or any one man. It is peculiarly in this kind of acquisition that the hospital clerk and the advanced student may be said to gall the heels of the oldest physician: and why should it not be so? What mystery is there, or what cumulative knowledge, demanding long study and research, in the tapping and listening we call percussion and auscultation? The truth is, that these processes require little or no mental effort, and are peculiarly independent of general professional attainments; so much so, that, the relative position and healthy sounds of the heart and lungs once acquired (and it is a mere mechanical lesson), a clever mechanic, without knowing any thing more of medicine, might become an expert auscultator. To be sure, he might not readily attain that faculty of minute diagnosis of which your stethoscopic enthusiasts are for ever boasting; but besides that great minuteness in physical diagnosis is generally useless for purposes of actual treatment, there are good grounds for suspecting that it is often either an imaginary gift, or fatal, in some incomprehensible way, to the *ordinary* observing powers of those who possess it. If the glaring mistakes, as well as the alleged refinements in physical diagnosis, of the loudest talkers, were allowed to transpire,

the public would be astounded to learn how brittle a weapon the stethoscope sometimes is, in the hands of a dogmatic physician.

In proof of this, we shall quote a passage from a work written, it is true, in defence of a system of which we are no disciple, but whose author is unquestionably a man of great ability, and second to none (whatever may be thought of his modes of treatment), in the detection of disease. We consider Professor Henderson's testimony on this subject as unimpeachable as it is significant: in his "Homœopathy Fairly Represented," he says:—

"I have *known* an accomplished consulting physician, and an eminent general practitioner, overlook or mistake double pneumonia of great extent, and discover it only on dissection; I have *known* a great advocate of cod-liver oil in consumption, mistake chronic pleurisy for the other disease; I have *known* an eminent stethoscopist, for mere irritation of the throat, which he treated with caustic as usual, mistake pulmonary consumption, which was fatal within the week, by the bursting of a tubercular abscess into the pleura. I have *known* an instance in which a notable hospital physician, not finding, on dissection, the pulmonary disease he had mapped out and described to his pupils, adroitly remarked, 'Gentlemen, you perceive the appearances on dissection don't correspond with the stethoscopic signs heard during life;' (the lung was sound)."

The above revelations will perhaps enable us to view in a different light from that in which Dr. Bennett has set it, the ensuing passage of his lecture (page 20):—

"I cannot too strongly caution you not to be influenced by the opinion of those, who, educated before these means of research came into general use, speak of them as worthless, especially in the investigation and diagnosis of disease—because, in short, they are ignorant of their value, therefore, forsooth, they can be of little benefit. I need scarcely remark that this kind of reasoning is altogether unsound, and is directly opposed to the introduction of all improvement in either science or art. What should we think of a modern astronomer who boasted that it was enough for him to examine the heavens with his naked eye, and sneered at telescopes? or how should we like to trust ourselves at sea to the navigator, who, as in ancient times, steered his course by the sun and stars only, and who abused sextants and other instruments by which alone exact results are arrived at? Such, however, is exactly the position of those medical men who consider stethescopes and microscopes useless, and thus betray an unacquaintance with the present state of their own art."

Unfortunately, at least for Dr. Bennett, the comparison here instituted between certain medical men who consider stethoscopes and microscopes utterly useless, and certain modern astronomers and ancient mariners who sneered at telescopes and quadrants, is a pure romance. There is no question among medical men, old or young now, as to the *uses* of stethoscopes and microscopes: it is only in the minds of the public that prejudices re-

arding these much talked of instruments can be truly said to xist. Every medical man knows that a stethoscope is nothing lse than a piece of wood, adopted out of delicacy or cleanliness; ot to assist the ear, but to keep the head and face of the aus- ultator at a respectful distance from the chest of his patient. t is, however, a popular idea—which those who are really ealous for the suppression of quackery should lose no time in orrecting—that, like the telescope, the stethoscope also is a cientific instrument, capable of bringing into notice what the aked sense is too limited to discern.

As to the microscope, its proper province is that of minute natomy, sound and morbid, not the practice of medicine. Its ccasional application to the purposes of the physician,—to the xamination of parts of the human body after death, or those roducts of the living body which have ceased to be vital at the ime of the observation (e. g. the blood and urine), is univer- ally admitted. On the other hand, it is allowed that micro- copical research can, of itself, no more make a man an accom- lished physician, than can the study of anatomical drawings make him a good surgeon; the former may, however, if too much harped on, have a tendency to narrow the mind, obscure the general intelligence, and rather unfit its devotee for the enlarged practice of his profession. There are certainly eminent men who think, and who have the courage to say what is not fashion- able at present, but not on that account the less true—that we run great danger by placing dependence on such appliances (for the stethoscope and microscope are no more than appliances), at the expense of other means of diagnosis much more valuable in kind and degree.

It has been the mode of late to underrate the importance of *symptoms* in disease, because they are matters of simple obser- vation and experience; as if there were any other constant guides, or general landmarks in practice. The stethoscope—that is, the ear—may often, it is true, remarkably assist in the diagnosis of a chest affection (although, as we have seen, it is very far from being an infallible aid), and the microscope throw light on a renal disorder; but symptoms can convey far more and reach much farther, for, besides being common to all dis-

eases, whether of the viscera, the blood, or the nervous system they often indicate the appropriate treatment, or at least lead to palliation where every thing else is doubtful. How often does it happen that nothing else can be done,—that the physician having nothing else to guide him, is compelled to treat the symptoms—and the symptoms of disease alone!

The most dangerous, because in some respects the most plausible, fallacy of modern medicine, is that which proposes to combat disease, in general, on scientific principles; such principles being, on the whole, quite inapplicable to an art of healing;—an art which must always depend mainly on observation and experience, and in proportion as it loses sight of these at any time, must lose ground, by forgetting or neglecting the lessons of the past, again necessary to be relearned. Thus it is that medicine moves, not in straight lines, but ever in circles; thus it is that the otherwise inexplicable fact becomes natural and consistent,—the fact, that, to-day—as it will doubtless always be more or less the case—we are reviving the remedies and the theories that fretted their brief hour, on the same stage long centuries ago.

That science shall much avail to conquer disease, is the dream of the enthusiast, or the specious tale of the march-of-progress charlatan: That medicine, with its admitted *no primitive fact* with its only possible definition of a *healing art*, should ever become an exact science, is as if one language should become common to all the tribes of earth, and one political government unite the nations of mankind in a universal and settled harmony. That disease with its changeful cycles, its startling revolutions, should ever stoop to the government of fixed laws, is as if the winds of heaven should again be confined in the cave of ÆEolus.

When chemistry* becomes alchemy,—when metals are readily

* Chemistry, we conceive, is destined to be the one strong point in the futu attempts that will be made (alas, in vain!) to erect a scientific scaffolding f medicine. The decomposition of *animal* gases will be successfully attempted large towns, as an important hindrance to the spread of fevers and cholera diarrhœas, because the attention of legislatures will at length be forcibly direct to the important subject of public hygiene; but chemistry can never be mad available in neutralizing vegetable or subterranean emanations, on the lar scale. In tropical and semitropical climes, slimy rivers with mangrove bank luxurious forests, and the rank vegetation of alluvial savannas and lovely b

ransmutable, and physicians cease to demand, because they no nger require, the fees of their patients,—then, but not till then, ill science and medicine form a rational and lasting alliance.

We have now done with Dr. Bennett's lecture,—a production, we make bold to say, remarkable for the narrowness of its iews, the absurdity of its illustration, and the ignorance it betrays of the first principles of ratiocination.

reacherous oases, will, for long centuries to eome, inevitably give rise to miasata, more or less fatal to human life wherever it may be wafted, on the wings f the wind, over this terraqueous globe. And when we eonsider that vast reions of Central America are yet but partially explored, while the whole of the normous Continent of Afriea, with the exeeption of a narrow inhabited coastelt, is still a *terra incognita*, probably consisting of alternate forests, mountains, swamps, and deserts—it beeomes at onee apparent that chemistry ean be of no vail in preventing, or even cheeking, those diseases we term pestilenees, infecious diseases, and epidemics. Moreover, the gases whieh issue from voleanie penings, craters and fissures in the earth's surfaee, probably are, and will conmue to be, a subtle source of mysterious diseases.

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